recd No 2 Ph. P. #16

S.W.C. 8th & Walnut

An inquiry

into

the secretion of bile.

By

L. D. Shepperd.

Hillsborough

N. Carolina

Passed March 6th 1826

Whether is

this subject

ally, or en

of the kidne

clear, ing

of urine.

The opinio

nested co

ical kn

ion of the

which are

such the

doctrine

cintly so

ially the

Whether is bile generated by the liver, or does it exist in the blood?

As a portion of the investigation of this subject will be analogical, & as this will consist principally, or entirely, in comparing the office of the liver with that of the kidneys, it appears necessary, that I should in the first place, inquire into the office of the latter in the production of urine.

How is urine formed?

The opinion most generally adopted & the one that has existed contemporary with the earliest dawn of physiological knowledge, is that it is formed by a specific action of the kidneys. But many facts & some experiments which are related in the annals of medical history, cause the correctness of this very ancient & popular doctrine to be somewhat questioned, at least sufficiently so to solicit a physiological investigation.

There are recorded by authors especially the more ancient many cases in which the secre-

tion of
through
other in
canal &
abound
ficiently
have b

course
in the
that, the
lar ca
First
perhap
vessels,
phatic
ward
which
Second

tion of urine was suppressed, & the urine instead of passing through its natural channels, was thrown off by some other emunctory or part, as the skin, ear, navel, alimentary canal &c. With similar cases our periodical works likewise abound, The explanation of which phenomena presents difficulties greater than would upon a slight view of the subject have been anticipated.

Dr. Fisher in his truly learned discourse on Paruria erratica, which made its appearance in the last number of the New England journal, observes that there are but three modes, by which these very singular cases can be accounted for.

First – The urine after it has arrived in the bladder, may perhaps be absorbed from that organ by its lymphatic vessels, & then be taken up from these vessels by other lymphatics which anastomose with them, & carried by a backward & retrogade action of these vessels, to the part ~~out~~ which it is to be discharged.

Second – The urine having been secreted by the kidneys

may possib
by them in
carried to th

be formed
my blood
similar to
naturally
to which
is formed i
of the kidne
duty, by g
it is throwo
intime vesse
the kidney
opendding
I will bre
Dr. Fisher
my opinio

may possibly be absorbed by the lymphatics & conveyed by them into the general circulating system, & then be carried to the unnatural outlets by the arteries of these parts.

Third — The urine may possibly be formed by the extreme vessels, which naturally convey blood, supposing them to take on a new action, or one similar to that of the extreme vessels of the kidneys, which naturally secrete urine.

To which I beg permission to add a fourth — That urine is formed in the blood, its formation being independent of the kidneys & that these organs refusing to perform their duty, by giving vent to it, subsequently to its formation, it is thrown off by some other emunctory, but without its extreme vessels assuming, the presumed specific action of the kidneys, its formation previously to reaching them, superceding the necessity.

I will briefly consider the merits of each.

Dr. Fisher argues the impracticability of the first, in my opinion, with considerable plausibility, for the requi

gitation
m of the p
supposition
inexplicab
Does it no
should be
through t
absorbing
able zigzag
pass throug
half of the

that the in
prove fa
the detecti
shall be
his general
to each of
of each of t

gitation of the original & ingenious Darwin, reminds me of the phlogiston of the ancient chymists - a convenient supposition for the solution of those phenomena, otherwise inexplicable.

Does it not appear extremely doubtful that the urine should travel from the bladder, to the ear (for instance) through the anastomoses of the extreme branches of the absorbents, to do which it must "pound in innumerable zigzag directions, & take a long circuitous rout, & pass through the extremities of the lymphatics of nearly one half of the body, in order to arrive" at its place of exit?

His objection to the second is that the introduction of urine into the blood would prove fatal. His reasoning on this subject is refuted by the detection of urea in the blood, instances of which shall be related in the sequel.

His general objection - that is the one - the one applying to each of the first, is very correct - by the inefficiency of each of them in explaining those cases in which

the urea
simple p

Either
tooth ye
scalp.
mucous
skin. Sh
of the at
their pro
passage a
If the
urine, li
instruce
same a
periated
tube or
sequence
nature.

the urine did not pass through the kidneys, cases verifying which, will be mentioned subsequently.

In the third hypothesis, to which Dr Fisher appears much attached & supported with no little zeal, an obstacle insurmountable, presents itself. That parts so different in appearance & in anatomical structure, as, for example, the kidneys & skin should either of them, take on the specific action of the other, an action as different from their own, as their products urine & sweat, appears too, improbable, to admit even of a supposition.

If the skin is thus adequate to the formation of perfect urine, where is the necessity of this notable difference in structure between the skin & kidneys? Why not the same organ, a tissue similar in formation, be appropriated for generating these fluids yielded by these organs? This certainly would be more simple, consequently more consistent with the general laws of nature. Besides, to admit Dr Fishers surmise, would be

gran t
apeum
but at
other c
be the c
proceſs
of urine
Whic
utility.
charge
the gas
here na
distaten

this th
the tra
third.
nomalo
case ind

granting a capacity in the extreme vessels, not only of assuming two specific actions, at different times but at one & the same time, which would surpass the other in point of impossibility & this must certainly be the case, unless there is an alternate suspension of the processes, while the skin is engaged in the generation of urine, there is a suppression of perspiration & vice versa

Which being the fact, the vicarious discharges, would be of little utility, since a suppression of any accustomed discharge, especially one as important as perspiration or the gastric juice, would be production of disease, and here nature would fall far short of perfecting her undertaking - viz. the prevention of disease.

But granting the possibility of this hypothesis, the objection alledged against the two first, is equally as applicable to this - the third. their inadequacy in the explanation of these anomalous cases, as evinced in the truly interesting case related, ~~related~~ in the dissertation alluded to.

Applicant p
caused by th
ign paralysis
to contents. Th
ch accident,
the nose or fa
ned which
ed of yellow
portion of pos
regularly, sh
hysterical co
upon the axis o
ney per pect
It pus sank
y painful.
charge of flui
came from th
ed from her
y navel, some

A coloured girl afflicted with a suppression of urine, caused by the harsh operation of an emetic. Her bladder was paralysed, & art is resorted to daily in order to vacuate its contents. The use of the catheter from some unavoidable accident, was omitted, for the space of forty-eight hours. She now experienced unpleasant sensations about the head, which was followed by a discharge of several ounces of yellowe fluid, from the right ear, possessing, the properties of perfect urine, this after a while was discharged regularly. She was afflicted at times with delirium & hysterical convulsions, which were more severe just before the discharge. A year after her attack, she vomited very perfect urine, in large quantities.

A few weeks after this, her breast became hot, distended & painful. These symptoms were followed, by a discharge of fluid, resembling in every respect, that, which came from the ear & stomach. In a short time, urine flowed from her left ear, afterwards from her left breast & navel, sometime intervening, between the discharges

the latter was preceded, by spasmodic pain & contraction about the umbilicus, & a noise similar to that produced by withdrawing a cork from a bottle. the surface of the patients body was almost the whole of this time bedewed with this fluid. It has also issued from the salivary glands & from the right eye. The right ear, nasal & breasts have none for three years but the daily outlet of this fluid, urine has also flowed, in different quantities into the bladder & has been evacuated by the catheter. When the quantities thrown out of these foreign outlets, was great, that passed from the bladder was small & vice versa. The hearing of the right ear & sight of the right eye is destroyed. portions of this fluid have been analyzed, & found to be perfect urine. The patient is able to ride, walk some distance & perform light work, & this without diminution, of the above phenomena. Blood drawn from the arm, & poured upon a heated shovel, gives out the peculiar odour of urine.

In this case urine is detected in the blood drawn from the larger vessels, & it cannot with reason, be said

that these vicarious discharges were formed by the specific action, assumed by the extreme vessels of the parts from which they issued, as a fluid similar to that of which they consist, has been discovered in the circulation, this case is certainly inexplicable by this hypothesis.

This case verifies the impossibility of the second, which is also inadequate, to the explanation of ~~this hypothesis~~ phenomena connected with it, as it would be unreasonable, to suppose, that such quantities of urine, could have been absorbed from the bladder, as was daily discharged by the foreign outlets, when this organ was kept empty by artificial means. Besides, we are informed, that in proportion to the accumulation of urine in the bladder, was the diminution of the unnatural discharges, which should not have been the case, were they dependent on this process. for is it rational to suppose, that the activity of the absorbents, would be diminished

by the in
as now
~~[illegible]~~
Moreover
in this pr
of the bla
I should
its place
fies of
folds,
The f

again
ity be a
deliteri
If urin
by a dyz
be produ
widely

by the increased accumulation of this fluid, which they are now destined to remove? Would they not rather be ~~increased~~ excited to greater vigour, by the stimulus of urine? Moreover, would not the number of these vessels engaged in this process, be increased in proportion to the distention of the bladder?

I should suppose that the corrugations of this organ, in its flaccid condition, would conceal many of the orifices of the absorbents, which the dessipation of these folds, by distention, would display.

The fourth & last hypothesis is now to be considered.

To this the objection alledged against the second, may with almost equal propriety be applied, viz. the impossibility of the existence of so deleterious a substance as urine in the blood.

If urine was suddenly thrown into the circulation, as by a syringe. I agree that it would more than probable be productive of death, but the consequences would be widely different when imptied into the blood vessels in so

gradual a
accumula
In these co
become by
ted a degre
the extreme
That it is
is very fou
have suffic
Tobacco, i
ably habit
as to those pr
luxuries, o
even pur
being, the
of habit, ab
distraging
spirituous
his stomach

gradual a manner, as by the absorbents & still more so when accumulated ~~by~~ the very gradual process of ~~degeneration~~ formation. In these cases it is so progressively formed, that the vessels become by degrees accustomed to the stimulus. Too exalted a degree of which, is prevented by the kind office of the extreme vessels of some portion of the system.

That it is possible for parts, thus to ~~be~~ become habituated to stimuli, very foreign to that, to which they are accustomed, we have sufficient evidence.

Tobacco, which to those, just embarking into the pernicious habit of its use, is highly offensive & disgustful is to those practised in its consumption, the choice of luxuries, such is the influence of habit, that this article is even permitted to enter the stomach, without mischief being the necessary consequence, which independent of habit, would exhibit symptoms the most ~~alarming~~ distressing & dangerous.

Spirituous liquors, which to the taper is the cordial of his stomach & the support of his frame, is to those marked

to go to sobrie
Opium to
incredible,
bothers in p
and symto
But let us
of the great
on by no m
all physicia
have contrig
I allude t
not only be
times a beso
vel of it op
cope. & in so
tion of the in

tions of auth
in favour of

for sobriety, production of effects, quite the reverse.
Opium taken by some individuals to an extent almost incredible, not only with impunity, but with pleasure, is to others in far less quantities productive of the most dangerous symptoms.

But besides, these more familiar examplifications of the great power exercised by habit over man, we have one by no means of an uncommon occurrence, one which all physicians, of a moderate degree of experience, must have witnessed & which is extremely pertinent.
I allude to the stimulus of distention in dropsy, which not only becomes, sufferable to the patient, but sometimes absolutely the support of the system, for the removal of it often causes relaxation, nausea & even syncope, & in some instances, evidently hastens the termination of the individuals existence.

There are cases, experiments & observations of authors recorded, which certainly evidence much in favour of this hypothesis.

There is a ce
pthly, dis o
pud. Yet the
from the pa
does made
approach
How wa
has winc
ith gen
tion, which
pouring
This evide
dent of the
can, is, m
cond se
ensum
ents', the
hem of the
to form

There is a case related, in which the kidneys were completely disorganized, being simply sacks containing pus. Yet there was a strong urinous odour arising from the patient. Here the formation or existence of urine was made evident, to the senses of all those, who might approach the unfortunate individual.

How was this fluid formed? Post mortem examination has witnessed these organs almost universally admitted as the generators of this fluid, to have been in a condition, which rendered them entirely incapable of performing such an office.

This evidently proves the formation of urine independent of the kidneys, & the phenomena attendant on this case, is, necessarily, inexplicable, upon the first & second supposition, as they are grounded upon the presumption, that this fluid has traversed these organs, therefore it can only be explained, by the admission of the third or fourth, & from the impracticability of the former, we are compelled to resort to the latter, for the

desired explanation. When the phenomenon, from being intricate & inexplicable, is rendered clear & comprehensible. The urine existing in the blood, independent of the specific action of any organ, has merely a vent given to it by some one or more of the emunctories.

P. But what strengthens this hypothesis & constrains me to confide in the correctness of it, is an experiment presently to be related, which proves more than a thousand speculations, which are most commonly the products of a fanciful imagination, destitute of facts for support.

M. Leglas (with professor Vauquelin, an ornament to his country & profession, distinguished alike for his talents, indefatigable labour, ardent zeal, & his valuable contributions to that branch of natural history to which he has devoted himself) discovered urea in the blood of a dog, whose kidneys were extirpated previous to his death, confirming what M. M. Prevost & Dumas of Geneva had announced.

Medical recorder. vol. 6th

This is a remar
stance of un
from which
by its reac
dust of any
been generat
found to be o
sign of the re
putrid. The
an explanati
season.

It is a
hypothesis,
mena, for the
as the last is
whole.

It may
dence of the
perspiration

This experiment evidently proves beyond a doubt the pre-existence of urine in the blood, that is - its existence in the blood from which it is supposed to be formed, by a specific action previously to its reaching the kidneys & also its formation independent of any secretory forgan, for it certainly could not have been generated by the assumption of this peculiar action supposed to be exerted by the kidneys, by any other organ, as the design of the vicarious discharges is to pu the blood of such impurities. The two first hypotheses are as deficient here in affecting an explanation, as in the case immediately preceding, & for the same reason.

It is apparent from what has been related, that all the Hypotheses, the last excepted, fall short in solving the phenomena, for the explanation of which they were contrived, whereas the last is admirably applicable to the explanation of the whole.

It may be maintained, that ursa is not a positive evidence of the existence of urine, as it has been detected in the perspiration of a horse, by Fourcroy & Vauquelin.*

* Edinburgh medical journal vol. 1st

A transitory view of this ~~of this~~, ~~subject~~ would be apparently sufficient, to render the experiment of M. Legallois futile, as an evidence of the formation of urine in the blood.

But reflection calls to the recollection this well established fact, that the production of urine & perspiration is in an inverse ratio - as one or the other is increased the other is diminished. Now for the procurement of perspiration in a sufficient quantity, for analysis, the horse must have been considerably exercised, which certainly would cause a determination of blood from the kidneys to the skin.

The skin thus receiving part of the blood, consequently part of the urea circulating with it, which is confined in a natural state of the system to the kidneys, now performs partly the office of the kidneys, as it evidently did in the case of suppuration of these organs, previously related.

This appears to be an additional evidence of the formation of urine in the blood, as from the frequent analysis of perspiration by the most distinguished Physiologists, we have no account of urea being one of its

constituent particles; & I presume never is discovered in it, except from some cause or other, the kidneys refuse to perform their duty, when the skin takes upon itself the libration of it from the blood, which is necessary to the preservation of health.

Many years previous to the performance of this convincing experiment by M. Segalas; it was asserted by the learned & indefatigable Haller (& although not verified by experiment, rendered extremely probable by observation) that urine was formed in the blood, & only separated by the kidneys. He defends his assertion by stating, that when its ordinary passage is obstructed, it is thrown off by skin, stomach & intestines &c. He relates cases, in which urine was discharged by the salivary organs, nostrils, mamma &c. mixed with blood. He has discovered it in the brain causing cephalalgia, blindness, delirium, stupor & apoplexy.

In some of these cases, the extreme vessels, could not have wrought the specific action on the blood necessary to the formation of urine, for blood unchanged was discharged & with it urine was mixed, & the third hypothesis again fails in effecting an explanation of these singular phenomena, & I resort to the fourth as the best adapted to the explanation, as having the greatest support from facts, experiments & observations, & as being one upon which all

these anomalous cases, can be accounted for, as will be shown immediately

Upon the whole I am led to believe — the urine to be formed in the blood, & the kidneys to act the subordinate office of separators — their occupation being merely to disengage the urine which is, as it were, the refuse of that fluid, the blood which has been appropriated to the nutrition of the body.

It appears to me — the blood in performing its duty, must suffer decomposition, & that portion which is adequate to the higher office of nutrition, is so employed, & that which is inadequate, is the residue, variously uniting so as to form perspiration, urine &c still circulating until it reaches those organs appropriated for their removal, as the skin, kidneys, &c.

These anomalous cases so plentifully recorded is thus explained. These vicarious discharges consist of urine, which is a portion of the fluid, the residue of the nutritions, thus accumulated in the system, & thrown off by the skin & other emunctories, in consequence of the ill condition of the kidneys, which would have removed it.

I now proceed to the inquiry, whether is there existing, the same relation, between the bile & liver, which appears to exist between the urine & kidneys.

the greatest objec
ing recomposed
that, while the fe
ing, to the suppo
tion agent in
tion.

But this
commonly co
highly requisite
a combination
is by actual e
chyme into chy
g, therefore the

eth, that the f
constant & re
luminosity of bu
entertainers are
burnings, which
forming our op
ble, viz. its in
occur in the pr
vegetative mot

Richeson

The greatest objection, which presents, as to the propriety of instituting a comparison between the office of the liver & kidneys, is that, while the product of one, is a recrementi fluid, not contributing to the support of the system, that of the other is considered an active agent in the promotion of digestion, consequently of nutrition.

But this office of the bile is not as well established, as it is commonly conceived to be. It is a received opinion that it is highly requisite in the promotion of digestion, that there should be a combination of bile, pancreatic juice &c with the chyme. Now by actual experiment it has been proved, that, the conversion of chyme into chyle is independent of these fluids. The ducts conveying them were tied, yet chyme was changed into chyle*.

It is necessary for the preservation of health, that the peristaltic motion of the intestines should be constant & regular; this is is termed impracticable with a deficiency of bile, by the application of which (it is believed) the intestines are gently stimulated to motion, thus obviating costiveness, which is a fruitful source of disease.

Forming our opinion merely upon the sensible qualities of bile, viz. its intense bitterness & acrimony, we might justly concur in the persuasion of its capacity in promoting the peristaltic motion of the intestines, for it is a fact established

* Richerand

by observation, that, in a healthy state of the system, any substance possessing but slightly stimulating qualities, will quicken this peristaltic motion of the intestines, as is exemplified in the operation of purgatives, in the action of which there is nothing peculiar, only differing from each other in their modus operandi, as they possess greater or less stimulating qualities.

Yet I cannot believe the intestines, except when in a pathological condition, perceive the stimulus of bile. It is so well known that purgs frequently administered, loose their effect, the intestines becoming habituated to their stimuli, have their sensibility blunted & cease to perceive them. I should suppose that this would be the case with bile, which is constantly applied.

To this it may be replied - There is direct evidence of the purgative quality of bile, the bile of the ox being one of the most efficient remedies in obstinate constipation. Besides this being the product of an animal very different from man, probably differing in composition, if not in the constituent particles, at least in the proportion of them, which circumstance renders the intestines, unaccustomed to this substance; this bile used as an evidence of the purgative quality of human bile, has been exposed to the atmosphere, which must have changed its properties, at least sufficiently to alter it in some degree from its healthy condition.

That the atmosphere possesses a tendency calculated to render substances, formerly innocious, highly injurious, when exposed to its action, we have evidence in the phenomena attendant on the opening of bloody tumours & large abscesses; the change which the air effects in the blood & the pus, renders them extremely irritating, producing violent inflammation, whereas had the air been excluded, they would have remained harmless; therefore I am persuaded - this circumstance is not inconsistant, with what I have advanced; this bile differing much from that the product of man, unexposed to the atmosphere, the intestines from habit insensible to the latter, feel the former & are stimulated to action.

As an additional evidence of the purgative quality of bile, may be advanced the phenomena accompanying cholera morbus, viz. puking & purging of bile. Here it would appear that bile was the exciting cause; that it does in these cases facilitate the peristaltic motion of the intestines, is indubitable; but this is not evidence sufficient to allow us to conclude, that bile exercises the same power constantly. The system is not in these cases in a condition, warranting the institution of such an analogy.

The liver in cholera morbus, is secondarily affected, the disease having its origin in the stomach, it is affected by continuous or vicious contiguous sympathy, & in consequence of this derangement, pours into the intestines, bile in excess of quantity & probably altered in quality, in proportion to the derangement of the system: the intestines although insensible to healthy bile, when applied gradually, become sensible to bile in excess of quantity, & especially when deviating from that which is salubrious.

Moreover; it is probable the intestines are in cholera morbus more irritable than usual, being affected in common with the system, & in some instances from the same cause, which first affected the stomach, passing from this organ into them, as when caused by crude aliment.

The cause of constipation is generally attributed, to a deficiency of bile, the intestines becoming sluggish in consequence of its absence. As far as my observation extends, a deficiency of bile is not commonly an attendant symptom on this state of the bowels. In all cases, which I have witnessed, clay, ash coloured stools, (the evidence of a slender supply of this fluid,) was absent, the discharges

being rather dark than otherwise.

In ~~the~~ support of the utility of bile may be advanced the clay colour of stools, being a symtom of ill health.
In these cases the deficiency of bile is not a cause of disease, but merely an effect, as in dyspepsia, in which the stomach is primarily ~~diseased~~ & the liver subsequently effected, hence in the first stage of this disease we are not presented with this symtom; but as the disease advances the liver becomes involved & the clay colour of the discharges is the consequence. So also in jaundice, which is not produced by a deficiency of bile in the intestines, but by its existence in the system.

It may farther be argued, that in certain diseases dark & tarry stools, are auspicious omens, & are by the knowing & feeling physician, hailed with gladness, while clay rash coloured discharges are evil symtoms, production of sensations as depressing, as the former were exhilarating.
In these cases the liver has been primarily or secondarily affected & the clay coloured ~~symtoms~~ stools are symtoms of the continuance of the disease, the dark & tarry of its solution. The deficiency of bile being, merely an effect, taken for a cause, too often the source of error.

That the function of the liver does not, diurnally, hold that rank in the animal economy which has been ascribed, is proven by the following case. "In opening the body of a man, who fell a victim to dropsy, a diligent search was made for the liver & spleen, of neither of which a vestage could be found. The substance of the intestines was extremely thick & fleshy, it was considerably more solid than muscle, so that in point of thickness it strongly resembled the structure of the heart. The vena cava arose from the intestines, nearly in the same way, that the vena portæ is situated in ~~lieu~~ regard to them."

Now if bile is so indispensably requisite, for the formation of chyle, the nutriment of the body. How was this patient, who it appears lived to be an adult nourished? According to the view of the subject usually received, the patient certainly would not have lived as many months but would have died from inanition; yet he was a man, to constitute which he must have been nineteen or twenty years of age at least.

The inutility of the bile appearing, to me, as probable as the uselessness of urine, having in its favour merely the circumstance, of its having passage through the intestines, which is certainly of little weight; for where else could it make

+ Lieutaud - observation § 15

its exit? this certainly seems to be the most convenient outlet; this interrogatory presents itself. Why should so complex an apparatus be provided for the generation of bile: while urine, which ~~is~~ bears an equality with it, neither of them contributing to the support of the system, except it be in a very indirect way — their absence preventing the inconveniences which would have been experienced by their presence, is the mere dregs of that fluid performing the office of nutrition: there appears to me no reasonable explanation.

In support of the opinion that bile exists in the blood, previously to the fluid, from which it is said to be formed, reaches the liver; we have the observations of distinguished physicians. Dissections likewise reveal much in its favour. Boerhaave & Morgani were of this belief.

An undue degree of excitement causes a suppression of the secretory ~~process~~, as is familiarly exemplified in the operation of purgatives. If the secreting surface of the intestines are over excited, & we administer cathartics, we are disappointed, in as much as, we have not the desired effect produced. the discharges from the bowels not being increased. From what does this disappointment proceed? From an ignorance of the existing condition of the intestines, which are

already stimulated, beyond what is termed the secreting point: by administering cathartics, in this state of things, we add fuel to the fire, & increase the tendency to constipation, as much as we increase the excitement, & remove the secreting point more remote; whereas were we, by any means, to abate the excitement, as by V.S. or by administering mucilaginous drinks instead of irritants, we might have produced discharges, thus reducing the excited intestines to the secreting point. We have another example equally as familiar - the suppression of perspiration, in consequence of the over excitement of the system.

Our treatises on diseases of the liver, abound with cases in which jaundice was produced, by an inflammation of this organ. The, advanced, explanation of which is - the ducti biliosi are compressed by the liver, which is in some degree indurated in consequence of the inflammation.

Judging from what we know of concerning the secretory process in those organs situated more externally, & exposed more immediately to our sights, I should suppose we must certainly conclude that an organ inflamed, especially to such a degree, as to produce

an obstruction of the biliary ducts, from its hardness, would not in this state secrete, & therefore pronounce this explanation unfounded, as it is based upon the supposition that bile is secreted. It would be more consistent with our knowledge of the laws of the secretory process to admit the non-existence of bile, (for it is known to every one capable of observing, & not confined to physicians alone, that an undue degree of excitement checks secretions, from the supposition that it is so) the following explanation is drawn — The liver in consequence of the too great excitement, does not secrete, & the bile remains in the system, imparting the yellow hue to the surface of the body, to which the appellation of jaundice is given.

The following case related to me by a highly respectable practitioner of this City favours this view of the production of jaundice from inflammation of the liver. It is a case of yellow fever, which occurred during the last summer. At first the secretions of perspiration & saliva were checked, subsequently there was a suppression of urine, immediately after which, the patient assumed a yellow colour, & in a little time was completely jaundiced. Post mortem examination, exhibited the ducts in perfectly a natural condition, & not a vestage of any substance, bearing the least appearance to bile, could be discovered

in any part of the liver; here successively there was a suppression of the secretions of the organs, exposed to our examination, as perspiration & urine, immediately after which, the patient assumed the yellow tinge, indicating the existence of bile in the system; certainly we have in this case the strongest circumstantial evidence of the suppression of the secretion of bile, & it does not seem unreasonable to attribute jaundice to this circumstance. The bile not being liberated coloured the surface of the body more especially, as dissection revealed no other cause, to which it might be imputed.

Saunders while enumerating the causes of obstructions to the passage of bile, which he considers as the cause of jaundice mentions "a schirrous, impacted state of the liver" with "a very extensive deposit of solid matter throughout its structure, in an interstitial form."[x]

Is it rational to suppose an organ in this disorganized state capable of secreting? It is generally believed that every gland effects a change, in the blood passing through it peculiar to itself, & thus the liver forms bile, the kidneys urine &c. If so this new organ (for such it is retaining none of the appearances formerly

Saunders on the liver

exhibited by the the liver, except form, & perhaps size), ought to have produced a new fluid, as different from bile, as it is from its original condition. (This remark will apply equally as forcible to some of the succeeding cases). Yet it is said, or something amounting to as much, that bile was secreted, & jaundice produced by the absorption or regurgitation, of it. In consequence of this ill condition of the liver obstructing its passage, this process was instituted by nature for its removal.

I would ask, which is the most compressible the ducts or extreme depths of the hepatic termination of the vena portarum? certainly the latter; then if the ducts were compressed & obstructed, a were partially so, by this condition of the liver, it would have been impossible for the blood of the vena portarum, to circulate through it, & certainly this would have been an end to the formation of any fluid & the phenomena rendered inexplicable, except the pre-existence of bile be admitted; the admission of which renders it comparatively easy.— This impacted, disorganized state of the liver, incapacitating it for acting the part of a separator; consequently the bile is not liberated from the system, & its accumulation constitutes jaundice.

Lieutaud, records the following case: "In a post mortem examination of a woman who fell a victim to chronic jaundice, in the forty eth year of her age, her liver appeared livid, hard, & perfectly dry; the gall bladder collapsed & quite empty; the stomach & intestines tinged with a livid hue, were distended with flatus, the spleen was larger than natural & somewhat putrid; the pancreas appeared stuffed & swollen; the epiploon was exceedingly thickened & sclirrus, with a copious yellow & stagnant serum. Even the bones were tinged in common parts of the system, with a yellow colour"*

In this case, as in the one succeeding, the liver was certainly in a condition which rendered the secretion of bile impracticable, especially to such an extent, as to penetrate every part of the system, "even the bones" But admitting that bile was secreted, the explanation of the production of jaundice, cannot be effected according to the received opinion. It will not answer to say - the ducts were compressed by the induration of the liver for the reason previously assigned - what would compress the ducts, would compress the extreme branches of the vena portarum. To this it may be replied the ducts were not closed completely; but in a greater or

* observation 317

less degree obstructed. The secreting vessels undergoing the same pressure, consequently more considerable closure; certainly could not have secreted with more rapidity, than the duct could have received. Besides, if the ducts were not completely closed, would they not have conveyed a portion of the bile, not a particle of which could could be discovered either in the ducts or gall bladder.

Tulpius mentions the case of a woman who died of jaundice & dropsy; her liver was found dry & black, without a particle of moisture, & closely resembling corrugated leather, such was its contractions, that it was scarcely the size of a double fist.

The size of this liver independent of its condition prevents the power of exercising the secretory action of a healthy liver, being attributed to it. What is said of the preceding cases is applicable to this.

A diminution or suppression of morbid vacuations, is occasionally a cause of jaundice.

Portal relates a case of ictirus, caused by a suppression of a morbid discharge from the axilla, which was cured by a return of it. This is a cause of jaundice, the modus operandi of

which cannot be explained, according to the common opinion existing relative to the production of bile.

It is indeed inexplicable, except the pre-existence of bile be admitted, when it is rendered easy of solution. Probably there was an unusual quantity of bile formed in the system of this patient, & the liver not being adequate to the separation of the whole, although in a perfectly sound state, & performing as much as would be required in a healthy condition of the system; the issue in the axilla, executed the kind office of extricating the remainder; thus while it continued discharging, jaundice was obviated, but as soon as it ceased, the portion formerly evacuated by the issue, remained in the system & constituted that disease, called jaundice.

This state of the system, in which so much bile is disengaged, is what I would understand Portal to mean by bilious plethora, which he makes mention of as a cause of jaundice.

Pregnancy is a frequent cause of partial & not unfrequently of universal icterus. Compression of the ductus communis, is generally thought to be its mode of action in the production of it. That it is so I will not positively deny, but when I consider the relation

situation, & anatomical structure of this duct & vena portarum, I am constrained to doubt it. I am inclined to believe, that, what would compress the duct, would compress the vein, the obstruction of which experience teaches us proves speedily fatal. The parietes of the duct is double the thickness of the vein & composed of a substance decidedly more unyielding. To this it may be replied, that pressure of the duct, sufficient to cause jaundice, would not impede the circulation in the vena portarum, to that extent, necessary for the production of disagreeable consequences.

In proportion to the quantity of blood received by the liver is the quantity of bile formed, this being the case, I should conclude since the vein is much more compressible, the capacity of the ducts for the transmission of bile, allowing for the pressure received by it, would be more than competent to the removal of it.

But I do not believe the pressure exercised ~~[illegible]~~ by the uterine tumour, in a place as elevated as the situation of the vein & duct, sufficient to compress either.

I have not observed very attentively, but as far as my observation goes, I am led to believe, that the base of the uterine tumour, has a tendency

to fall anteriorly, & rest against the abdominal muscles, & consequently there is proportionably a tendency of the apex to approach the spine, & agreeable to this, the pressure is increased as we extend from above downwards, & the pressure at the inferior part of the abdomen, being considerable, while there is comparatively none at the superior; the situation of the duct & vein.

I believe pregnancy proves a cause of icterus, from the pressure of the uterine tumour, on the mesenteric veins, previously to the formation of the vena portarum; the circulation being thus impeded, only part of the bile, formed, reaches the liver, & the remaining portion imparts the the yellow hue, the frequency of which, has rendered it almost characteristic of a pregnant woman, & is sometimes so abundant, as to acquire the appellation of jaundice.

The Oedema to which pregnant women are subject, is an evidence of the uterine tumour, impeding circulation in the abdominal branches of the vena portarum; they veins in consequence of the pressure, do not receive the blood from the arteries, as freely as necessary, & the arteries relieve themselves by effusion.

Portal believes in fact the jaundice to be caused by clos-

sure of the hepatic vessels. This is extremely probable, the closure of these vessels for a time impeds the circulation, & is the source of jaundice, which continues until the infantile circulation is completely & rightly established, when it disappears.

It is the existing opinion, that the hepatic artery nourishes the liver, & if this artery be tied, its only source of nutriment is cut off, & it must perish. Yet this vessel has been tied & bile continued to be secreted. If the bile was formed by the specific action of the liver, the formation of this fluid certainly ought to have ceased with its death, but there was nothing, immediately, to prevent the separation of bile already formed.

If I am not mistaken, it is a fact known to almost all practitioners, that urine is secreted in far less abundance, when there is an obstruction to its free outlet, & that if the obstruction continues, it almost or entirely ceases, as occurred in Dr Fishes case previously related.

From this circumstance, I am inclined to believe it probable, that in the cases of jaundice caused by obstruction in the ducts, jaundice is not produced by absorption or regurgitation of bile. but that the bile which has passed through the liver remains in the ducts, & that in consequence of the impediment, which the bile meets with, in its outward

passage - the liver from some mysterious, ~~but~~ well known cause, refuses to transmit any more, which remaining in the system produces jaundice

In conclusion I will mention, that, I endeavoured to discover on which side of this question truth rested, by experiment; I tied the vena portarum in several cats, (which animals I chose as being extremely tenacious of life) with the view of producing jaundice if practicable, but the operations proved so speedily fatal, that I could not profit from them. Because of the want of time, & the inconvenience of obtaining them, I did not attempt to perform the experiments on any of the cold blooded animals; which I ~~(in)~~ inclined to believe would bear the operation.

.5

a cause

Tho